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duces our experience and justifies our estimate of it, and without which as rational beings we could not continue to have the experience. If theology is to be stated in terms of life rather than in terms of thought, it would seem to be an abandonment of his attempt to solve the problem. The two cannot be compared, and they are not to exist apart. We certainly can heartily agree that theology should be concrete and for our own age; but it should never be distressingly indistinct or elusive. It is always primarily theory and not practice, and needs no apology for it. It is enough that it be correct and scientific theory, and lead into wisely devoted life. And we hardly need to be reminded that life, after all, is not muscular but mental. To imply, therefore, that the scientific grasp of truth is of less importance than the practical activities of the every-day Christian, is the practical desertion of the theological field and the making of Christian sociology the goal of all our attempts to attain to the Christian idea of God and the world.

Dr. Brown does not mean this, but by going beyond his historical study, which is certainly a valuable one, and then failing to give something definite and conclusive, his book seems to run out into the mists and shadows. The only adequate explanation would seem to be that he has intended this volume simply as an introductory study, which he purposes to follow by another that shall settle all these questions from his own point of view. We need not assure Dr. Brown that we shall give such a book a hearty welcome.

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#### THE EXCAVATION OF NIPPUR.<sup>1</sup>

In the concluding chapter of *Nippur*, published in 1897, are these words:

How successfully Mr. Haynes carried out the work which he had planned, in his long and arduous excavations, covering three years, . . . and what wonderful treasures he unearthed, he has related in another volume. (Vol. II, p. 372.)

The reference was to a volume by Dr. Haynes, the manuscript of which was reported at that time as complete, and which was announced by

<sup>1</sup> *Explorations in Bible Lands during the Nineteenth Century*. By H. V. HILPRECHT, with the co-operation of LIC. DR. BENZINGER, PROF. DR. HOMMEL, PROF. DR. JENSEN, PROF. DR. STEINDORFF. Philadelphia: Holman & Co., 1903. xxiv + 793 pages. \$4.

Wattles & Co., of Philadelphia, the publishers of the *Sunday-School Times*, as about to be issued by them. This volume was for some reason withdrawn, and now, for the first time, in *Explorations in Bible Lands during the Nineteenth Century*, we have from Professor Hilprecht's pen, a general statement of the results of that expedition, and of Haynes's second expedition, 1899-1900, during the last two months of which Hilprecht took charge of the work in the field. In addition, Hilprecht's narrative undertakes to cover also the work previously done under my direction in 1888-90. In other words, this volume contains a résumé and general presentation of the work done at Nippur and the results achieved from 1888 to 1900. Dr. Hilprecht makes so severe a criticism of my work and that of Dr. Haynes that, before proceeding to discuss the archæological results achieved, I am obliged to consider his statements as to my methods, and also those of Dr. Haynes, the responsibility for those methods, and Dr. Hilprecht's own part in the expedition, its plans and its achievements.

I have related in my *Nippur* the inception of the undertaking, which took formal shape in a meeting held at the house of Provost Pepper, November 30, 1887. Dr. Hilprecht commences his story of the expedition with that meeting (pp. 297 ff.). My plans were of the simplest, since it seemed impossible to obtain funds for a larger enterprise until it had been shown that it was practicable, not only to excavate in Babylonia, but also to secure from the Turkish government a reasonable portion of the objects found. I had been trying for some years to arouse interest in excavation in Babylonia, and had encountered a general unwillingness to contribute money without a good prospect of "tangible results." We were doing pioneer work, and it was necessary to make a success on a small scale before we could hope to obtain large contributions.

My plans, as formulated and presented at that time, are not quite correctly represented in Dr. Hilprecht's volume, as can be seen by reference to my own statements in the work referred to above. Dr. Hilprecht claims to have opposed my plans as unscientific and proposed a more elaborate scheme. I have no recollection of such representations on his part at that meeting, and my recollections in this point are confirmed by others. Certainly Hilprecht played no evident part in the matter. A more elaborate plan was presented a little later, but until the appearance of the present volume I had supposed that it originated with Professor Haupt, of Baltimore. This more elaborate plan proposed a larger scientific staff, which was by no means unwelcome to

me, provided the funds for the purpose could be raised. On the larger staff which was ultimately adopted, Dr. R. F. Harper, then instructor in Yale University, was appointed Assyriologist. This was from no desire to slight Professor Hilprecht, who was at that time my colleague in the faculty of the University of Pennsylvania. It was entirely on my recommendation that he had been brought to this country a short time before, and it was naturally my interest that a person so recommended by me should receive due recognition. Dr. Pepper, then provost of the University of Pennsylvania, had assured me that Professor Hilprecht's health would not allow him to endure the hardships of a campaign in the field, and Dr. Pepper and I had planned for him a position of dignity and importance in connection with the work at home, first as secretary of the committee, and secondly as the person to whom should be assigned the work of publishing the cuneiform records obtained by the expedition. Somewhat later Dr. Pepper told me that Professor Hilprecht felt so chagrined at not being appointed on the staff of the expedition that he believed it would be worse for his health not to go than to go, and asked if we could not make a position for him. With the consent of Dr. Harper, Hilprecht was accordingly appointed on the field staff as a second Assyriologist (p. 300) and accompanied the first expedition, which conducted excavations for a period of about ten weeks.

On this first expedition, according to his own account, Hilprecht seems to have determined satisfactorily the topography of the mounds and to have identified the location of the library (pp. 306-9). I cannot help thinking that he has recollected forwards. At least neither I nor any of his other comrades with whom I have been able to communicate recollect any such identifications as he describes. Outside of the identification of the site of the *ziggurra*t, which was made by all of us on the ground of the prominence and the curious cone shape of the hill called Bint el-Amir, the only topographical suggestion made by Professor Hilprecht which I clearly remember was a theory based on the results of our earliest trenches, namely: that the complex of mounds to the east of the great canal dividing the ruins constituted "the city of the living," and those to the west "the city of the dead." Like much of our early theorizing, this was soon proved to be false.

The "temple library" was finally found by Haynes, in 1899-1900, in the northeast corner of the isolated triangular mound at the extreme southern end of the eastern half of the city, hill No. IV, according to Hilprecht's lettering in the present volume (p. 305), No. V in my

*Nippur* and in Hilprecht's *Babylonian Expedition of the University of Pennsylvania*.<sup>2</sup> Some chance trial trenches conducted by us, in the first expedition, in the extreme northwestern nose of that mound, not at Hilprecht's special request, as his memory now says, resulted in the discovery of a considerable number of tablets and fragments of tablets; but none of those were then identified by the Assyriologists connected with the expedition as possessing a literary character. They were reported to be contract tablets, partly of the Persian and neo-Babylonian period, and partly of the period of the first Babylonian dynasty, commonly called that of Hammurabi. It would seem from Hilprecht's statement that at a later date he discovered among these fragments some literary remains. This discovery must have been made, I should suppose, later than the date of Haynes's first expedition, and indeed Hilprecht's statement in the present volume is the first information I have received that there was such material in the discoveries of the first year. Writing to me under date of October 8, 1889, he says: "The only good things [discovered by us in the first campaign] are the text of Naram Sin (three lines) and the astronomical tablet;" neither of which, so far as we know, were discovered in that mound. The Ashur-etil-ilâni tablets, referred to by Hilprecht (p. 310) as found in the excavations in this hill and as possessing unusual historical interest, were also excavated in a different mound, No. VI in my *Nippur*, at the extreme north of the complex of mounds on the east side of the canal. Owing to the discovery of tablets in our first campaign, I conducted during the second campaign much more extensive excavations at various points in mound IV, finding, on the western side of the mound, along the edge of the great canal, a large number of tablets, almost entirely of a business character, dating from about 2500 B. C. on to the Persian period. Trenches in other parts of the mound produced no result. Among these tablets of the second year also, Hilprecht, with whom I was in constant communication with regard to the objects found, does not seem to have discovered any literary remains up to the close at least of Haynes's first expedition, and this hill was not, accordingly, included among the places especially recommended to Haynes for excavation during the years 1893-96. In the instructions which Hilprecht reports as given to Haynes for his second expedition, 1899-1900 (p. 430), the examination of this mound is included, and I suppose, therefore, that by that time Hilprecht had

<sup>2</sup> It is not clear to me why, in the present volume, Hilprecht has changed the etymology formerly adopted, thus causing confusion in the comparison of results.

found, in examining the tablets, the literary material which he refers to in this volume. The deposit of tablets described as a "temple library" was actually found by Haynes, on his second expedition, near the northeast corner of this mound, as stated above, at a depth of from twenty to twenty-four feet below the surface. Haynes, temporarily unable to continue work in the temple, had placed his men at the nearest opposite point on this hill, and there discovered the tablets. The discovery was made at the very end of 1899, and the tablets containing the library had been excavated before Hilprecht's arrival, March 1, 1900.

Hilprecht regards this entire mound as constituting a temple library from the period of 2500 B. C. onward. It is not at all clear that this is the case. As already stated, the tablets found by me on the western edge of the mound along the canal were almost entirely of a business character, and nowhere was there any large deposit of tablets. There were no rooms in which the tablets had been arranged upon shelves. The only part of the mounds of which that is true is the small section in the northeast, opposite the temple gate, and at the time when Haynes stopped work at this place he seemed to have exhausted the "find" of tablets. Hilprecht speaks also of the discovery of a section of the "library" on the west side of this mound (p. 512), but his further statements as to Haynes's failure to keep records and the uncertainty as to the place in which objects supposed to belong to the library were actually found, raises a question about these discoveries until we have more certain data; which, as I read, we have at present only for the northeast corner.

It will be observed that in Hilprecht's present reconstruction of the topography of Nippur (p. 550) he holds that the two great walls, Imgur-Marduk and Nimit-Marduk, surrounded only the temple complex. These two walls he believes have been discovered in the great wall surrounding the immediate temple inclosure, and an outside wall surrounding the entire temple complex. Now, this latter wall includes only that portion of the ruins on the east side of the canal north of this library hill. It certainly seems strange that, if the buildings on this hill were the temple library and temple schools, they should have been left outside of the area inclosed within the outside temple walls; and also that they should have been separated from the temple by a broad canal. One naturally awaits with much interest the further examination of this mound, but the results so far obtained do not yet appear, I think, to justify Hilprecht's conclusion that the whole

thirteen acres of this mound constituted a temple library and temple schools.

But to return to my *apologia*. Hilprecht represents me as desiring on my second expedition to conduct the excavations alone, unhampered by expert advice, and as having no interest in any other work than the collection of portable material, especially tablets, without regard to constructions and the like (pp. 320, 321). The first year's work had ended in catastrophe, and was regarded, or appeared to be regarded by my comrades and by the world at large, as a failure. I have already quoted one paragraph from a letter from Professor Hilprecht regarding the paucity of the first year's results. In the same letter he says that everywhere among scholars he hears the report that the first year's expedition was a failure; and expresses his regret that I am to resume excavations at Nippur, which he had hoped would be abandoned in favor of another site, preferably Mughair (Ur). Every member of my staff had resigned, a large sum of money had been spent in excavating at Nippur<sup>3</sup> with no tangible results, and the gentlemen of Philadelphia were naturally dissatisfied. I was recalled for consultation with the committee, and the first question asked me by the treasurer was, how much it would cost to settle the whole matter and end the expedition. Furthermore, it was very uncertain whether the Turkish government would allow us to return to Nippur at all. With this uncertainty and the difficulty of securing funds, resulting from our failure to produce results, it was natural that the committee should be unwilling to send a large staff into the field, which might spend months at Aleppo or some other place, waiting for permission to proceed, as we had done in the first campaign. It was not my desire to conduct the excavations alone, but the necessities of the situation, which made the committee conclude that it was undesirable to provide me with a staff. It was even against the written recommendation of the committee that I engaged Haynes for the second year in order to secure at least the advantage of photography. I had originally become the director of the expedition, because in that way only did it seem possible to secure the funds for the work which I wished to see prosecuted in the excavation of old Babylonian sites. I had originally gone out for one year only, but before the first year was up the committee had advised me that it was absolutely necessary

<sup>3</sup> Hilprecht's statement of the amount expended (p. 318) is, however, some \$6,000 in excess of the actual amount, as can be seen by a reference to my *Nippur*, Vol. I, p. 296.

that I should continue in the field a second year; that otherwise the work would fail, as support could not be secured. After the failure of the first expedition I felt a personal responsibility to repay the faith and the expenditures of the committee, as it were, by turning failure into success. I felt keenly during the second campaign the lack of an expert architect or engineer, and it was that which led me to avail myself of the opportunity, to which Hilprecht refers (p. 344), to secure for a time the services of a Hungarian engineer, formerly in the employ of the Ottoman government.

In a note on p. 339 Hilprecht denies my statement that the committee in Philadelphia made a constant demand for objects. My account of my work in *Nippur* will show anyone who reads that book that Hilprecht's representation that my only thought was to search here, there, and everywhere for tablets is a misrepresentation. The bulk of my workmen were concentrated on the exploration of the temple construction, which certainly was not a search for tablets. Another considerable body were occupied during a great part of the time in the excavation of the next most interesting building found in the mounds, the Parthian palace on the west side of the canal. Gangs of selected workmen, as I have stated in my *Nippur*, were employed by me in search for tablets, though not quite in the method described by Dr. Hilprecht. I was, of course, very much interested in the discovery of tablets, both for themselves—for inscribed tablets are, after all, the most valuable discoveries made in Assyria and Babylonia—but also, and even more particularly, because the whole future work of excavation in Babylonia seemed to depend on my success in finding and obtaining for the museum in Philadelphia large quantities of inscribed material. Here is a quotation from a characteristic letter of the chairman of the committee, under date of February 10, 1890:

All depends upon this year's work. You must make large finds, and Hamdy Bey must make a liberal division of them, so that we can have important collections in hand before summer.

My hope was to make such a success of the second campaign as to place the enterprise of excavating Nippur on a secure foundation, so that I should have successors who could excavate completely all, at least, that was important in the mounds of Nippur. In the work on the temple, and, indeed, in general in the work of excavation, I was a pioneer. Our trenches in the first year were entirely tentative, and a large part of my work in the second year was of the same character. I was endeavoring to find where the remains were which should be



excavated, and above all to discover if there were important ancient remains underneath the late structures and the huge mass of late débris on the temple mound. It should be remembered also that it was very uncertain whether the work would be continued at all. I had, therefore, not only to prepare the work for the excavations which were to follow, but also so to excavate that, if the work were abandoned, we should, if possible, know something of an old Babylonian temple. I am not claiming that I did not make very serious mistakes. It would have been strange if a much wiser man than I should not have done so under the same circumstances. It would be strange, also, if, considering the character of the remains of the temple, unlike anything previously explored, as Hilprecht points out, and composed of unbaked brick, I did not destroy some things and fail to understand much more. I can, however, claim that I succeeded in discovering where the most important remains were to be found, and put my successors in the way of excavating them scientifically. For the proof of this I must refer my readers once more to my *Nippur*.

Hilprecht seems often to go out of his way to criticise my work, sometimes with rather ludicrous results. So, for instance, he blames me (p. 332) for my "endeavor to reach the older remains before the more recent strata had been investigated" by means of a diagonal trench cut through the center of the *ziggurra*.<sup>4</sup> Now, it chanced that this particular trench was a part of that "systematic exploration of Bint el-Amir . . . undertaken in accordance with a plan prepared by the Assyriologists and the architect" (p. 308) in the first year; so that according to his own showing Professor Hilprecht was in this matter *particeps criminis*. The same is true of a tunnel beneath the large court of the Parthian palace on the west side of the canal, which he criticises in a similar manner (pp. 567 f.).

Having made a success of the second expedition, to the extent that I had proved that work could be conducted at Nippur and that the results to be obtained were very great, I felt that my part had been accomplished. I was urged to continue for a third year, but could not arrange to do so. The difficulty was to find someone to take up the work. I recommended Haynes, my lieutenant in the second expedition, for that purpose, and if it had been possible to

<sup>4</sup> It should be said, by the way, that no later excavations would have been conducted at Nippur unless, by means of wells and shafts, I had first ascertained that underneath the immense accumulation of late material in the recent strata there lay older remains worth examining.

transport the objects found to Constantinople and secure a liberal portion of those objects within any reasonable time, the expedition would have been continued without interruption. Haynes did in fact remain at Baghdad for some time waiting for instructions from the committee. But the committee was unwilling to go on, and indeed felt itself unable to raise the money to do so, until it had some tangible results to show to the contributors. I left Nippur in May, 1890, but it was not until the close of the year 1891 that the objects assigned to us began to reach Philadelphia. The result of this long delay was that enthusiasm had cooled, and the expedition under the first *iradé* was allowed to come to a close.

Then followed what was to me a very discouraging period. It was proposed to abandon the work at Nippur, which, from my point of view, had only just begun, and to confine ourselves to the study and publication of the material discovered. Money was needed for museum purposes, to send Hilprecht to Constantinople to study the objects retained there and to publish the material in proper form, and for a time it seemed as though no money could be obtained for the work in the field. It was not because I, or presumably the committee, felt that the best method of excavation was to have only one man in the field that Haynes was sent out alone in 1892. I believed then, as I believe now, that if we did not continue the work at Nippur at once, the greater part of what we had accomplished would be lost, not merely nor chiefly through illicit digging by the Arabs and the consequent destruction of the ruins, but by the loss of interest at home, which would have resulted in the complete abandonment of the enterprise. Haynes was the only available man to send. He was not the ideal man, but he had had a long experience in work, and no one else could be secured; for both at this time and later the greatest difficulty was found in obtaining anyone to send to conduct excavations at Nippur. With much misgiving, but believing it to be a necessity of the work, Haynes was sent out alone to do what no one had done before — to conduct excavations continuously, winter and summer. A peculiar responsibility was also laid upon me for the success of Haynes's work, which was undertaken largely at my urgency.

Hilprecht represents Haynes's work as having been a mere search for tablets, and holds partly Haynes and partly me responsible for this. The instructions which I drew up for Haynes, as reported in very brief form in Vol. II, pp. 371 f., of my *Nippur*, should show, I think, what was my attitude with regard to the excavations. Hilprecht

states (p. 353) that Haynes abandoned the work on the temple mound "and undertook to unearth a sufficient quantity of tablets to meet Peters's growing demands for inscribed material." This demand came not from me, but from the committee. Under date of September 2, 1894, I received a letter from the chairman of the committee which contains this statement :

The work done by Haynes, during the past few months, in excavating the ancient temple, is very interesting; but I would like to have some portable finds.

He goes on to add that he will write him to send a force of men to excavate for tablets. Haynes had written on June 30, asking permission to work for not exceeding two months on the temple, and that time was up. Hilprecht quotes (p. 370) from one of Haynes's reports about this time :

I should like to see systematic excavations undertaken on this temple enclosure, not to be excavated section by section, but carried down as a whole, to distinguish the different epochs of its history, each well-defined level to be thoroughly explored, sketched, photographed, and described, before the excavation of any part should be carried to a lower level. This method would be most satisfactory and less likely to lead to confusion of strata and levels.

To which Hilprecht adds :

We naturally ask in amazement : Though knowing the better method, why did he never adopt it at a time when he was in complete charge of the expedition in the field, and the committee at home ready to support him with all the necessary technical assistance?

The above letter, and others of a similar tenor, will explain at least the reason why he left the temple to search for tablets, and will show the attitude of the committee, of which Hilprecht was a member, at that time.

Haynes achieved most remarkable success in his search for tablets, and if the explorations of the University of Pennsylvania at Nippur are now established on a secure and scientific basis, it must be said that this is largely, if not chiefly, due to his success in that regard. At that time also Hilprecht appears to have regarded his work as scientific. So he writes at the close of Haynes's excavations in 1896<sup>5</sup> that his work "is equal to that of Layard and Victor Place in Assyria and something without parallel in previous expeditions to Babylonia." Hilprecht was at that time aware—or should have been aware, for he had access to such reports as existed—of the nature of Haynes's work.

<sup>5</sup> *Babylonian Expedition of the University of Pennsylvania*, Vol. I, Part II, p. 9.

Haynes's position during that long and lonely period of this first expedition was an extremely trying one, physically, mentally, and morally. As Hilprecht has stated, he failed to make intelligible reports, or to keep records, so that it was impossible to determine what work he had done, what had been excavated, where the objects found had been discovered, etc. The strain and the climate had told upon him severely, he became morbid and suspicious, and when at last Duncan and Geere were sent out to Nippur he drove them away. These things, hitherto unpublished, Hilprecht has thought fit to reveal. But the revelation makes his own course in urging Haynes on the committee as director of the fourth expedition, and commending his methods as equal to those of Layard and Place, the more inexplicable. It was entirely through Hilprecht's urgency, against my earnest protest, that Haynes was sent out the second time (1899-1900). I resigned from the committee, and my knowledge of the results of the last expedition is, therefore, derived entirely, or almost entirely, from the statements contained in Professor Hilprecht's present volume. Hilprecht says that Haynes was more successful than ever in finding tablets, but that he kept no records, made no intelligible reports, and prevented the architects who had been sent with him from doing anything, so that it finally became necessary to send someone out to supersede him and bring order out of the chaos. The person sent was Dr. Hilprecht, who occupied the position of "scientific director" of this last expedition, and who spent a little more than two months, from March 1 to May 4, on this, his second visit to Nippur. I do not wish to minimize the excellent work which Dr. Hilprecht has done, but it does not seem fair that he should throw on Haynes's shoulders the burden of all failure and claim all success for himself.

So much for the narrative; and now for the statement of results achieved; and first some criticisms of comparatively minor details. The terra-cotta cones, to which Hilprecht refers on pp. 311, 312 as found in the first campaign along the base of the northwest wall of the *ziggurra*t, were in reality found along the base of an outer wall of brick at what appeared to be the extreme northern corner of the whole temple inclosure. This wall belonged to a series of brick buildings which were only partially excavated in the first and second years, and which do not seem to have been touched in the later campaigns of Haynes and Hilprecht. The place of discovery of these cones may be a matter of importance in determining the date and purpose of these structures. The place of finding the marble tablets containing a list of garments

presented to the temple, mentioned on p. 312, was, apparently, not the temple, as Hilprecht states,<sup>6</sup> but an outlying mound some distance to the south of the temple. The place was not determined with absolute certainty, inasmuch as this tablet was among the objects recovered from tablet thieves. The objects found by me in my second campaign in the great trench to the southeast of the *ziggurra*t and the levels of those objects are incorrectly stated by Hilprecht (p. 333; see *Nippur*, Vol. II, p. 159).

The results of the later explorations, as interpreted by Dr. Hilprecht in this volume, reverse, in not a few particulars, theories put forth by me in *Nippur*, or by him in the volumes of *The Babylonian Expedition of the University of Pennsylvania*. Some of these reversals are, I think, clearly proved. Of others I am not so certain. Among the most interesting "finds" of my second campaign was the so-called "jeweler's shop," in a low line of mounds to the southeast of the temple inclosure. As all the objects found in this shop belonged to the later Cassite kings, I supposed that the buildings in which they were found were of that period also. Hilprecht now holds that these objects were part of the stock of a jeweler of the Parthian period, a man who gathered old fragments and converted them into beads and the like, and that, accordingly, the buildings in which these objects were found were Parthian and belonged to the last reconstruction of the buildings on the temple ruins. One or two similar collections he reports to have been found in the last expedition to Nippur and in the German expedition to Babylonia. His conclusion may be correct, but it should be observed that the collection found by me differs from the similar collections reported by him as found elsewhere (p. 335) in that, while those collections consisted of material from different ancient periods, the material found in my collection was all from one period, the later Cassite. Moreover, there were here blocks of crude material, lapis lazuli, magnesite, malachite, etc., which were entirely unworked, side by side with completed and half-completed objects made from the same material. One object was evidently in process of manufacture as a tablet with a Cassite inscription. It was a rough block of lapis lazuli, one side of which had been smoothed and polished. On this side an inscription had been incised, after which the manufacturer had begun to saw off the inscribed face as a tablet. It is noticeable that the box in which this collection had been inclosed was fastened with copper, not iron, nails, which again would seem more appropriate to the Cassite than to the Parthian period.

<sup>6</sup>He had correctly located this tablet in a former work.

The building about the court of columns on the west side of the canal, to which I devoted a chapter<sup>7</sup> and which, with much uncertainty, I ascribed to the Cassite period, appears to have been fully or more fully explored under Professor Hilprecht's directions in the last expedition,<sup>8</sup> and we have a ground plan of the entire structure on p. 567. Hilprecht seems to have determined satisfactorily, by coins found in a brick, that this building, which was only partially excavated by me,<sup>9</sup> was Parthian and not Cassite.<sup>10</sup> On examining the plan and accompanying description in Hilprecht's volume, I find myself somewhat embarrassed by the fact that there is nothing to show certainly whether the whole building was excavated and the walls found as given on the ground plan, or whether some parts of the plan are a speculative restoration. I found a deep gully covering a portion of the space occupied by this building on the northeast and southeast. Water had washed away the earth to a depth far below the foundations of the building. I should like, also, to know the grounds on which the very large round column before the court<sup>11</sup> is identified as an altar. In the room marked 16 on Hilprecht's plan, which is described thus:

a kind of anteroom formed a connecting link between the men's quarters, the servants' rooms, and the section reserved for the women,

I found a large store of burned barley. The whole room was full of it. The conditions of excavation and the exact facts with regard to the discoveries made in this interesting little building will doubtless be given in some future volume, with statements sufficiently detailed to enable us to check the plan presented, which it is impossible to do with the material contained in the present volume.

On p. 559 Hilprecht gives a plan of a Parthian fortress, which shows, so far as the actual excavation of the Parthian remains on the top of the temple mound are concerned, little advance on the work done by me in the second year, and shown on the plan of excavations facing p. 142 in Vol. II, of my *Nippur*. In fact, this is substantially my plan differently shaded. My plan also shows the excavations and the remains of constructions at lower levels, while Hilprecht's represents only the buildings at the highest level. In this case also he

<sup>7</sup> Vol. II, chap. 6.

<sup>8</sup> Cf. pp. 336 f., 563 ff.

<sup>9</sup> See my plan, *Nippur*, Vol. II, p. 178.

<sup>10</sup> His statement, by the way, of the grounds of my suggestion that the building was of the Cassite period, cannot fairly be said to represent the argument used by me, which can be found in the chapter referred to.

<sup>11</sup> No. 5, on Hilprecht's plan.

seems to have found satisfactory evidence of the date and purpose of the structure, which I did not.<sup>12</sup> Hilprecht holds that this building was in occupancy as a fortress at least as late as the first Christian century, and adduces as proof a very interesting tomb found under one of the rooms on the outer wall of the fortress (p. 507), in which was a gold coin of the Emperor Tiberius. In general a tomb found among the remains of any construction is not an evidence that that construction was in use, but that it was in ruins, at that period. Two other brick tombs of a somewhat similar description were found by me among the ruins of the buildings of this latest reconstruction.

My excavations and those of Haynes had shown that there had been a destruction of temple property, and apparently of temple buildings, at a certain period, which period seemed to me to coincide with the period of the supremacy of Babylon. Furthermore, I found no evidence, in the way of inscribed bricks and the like, of the reconstruction of the temple by the kings of Babylon. In view of the rivalry existing between Bêl-Enlil of Nippur and Bêl-Marduk of Babylon, I therefore reached the conclusion that the destruction of these objects was due to the Babylonians.<sup>13</sup> Hilprecht, in his *Babylonian Expedition of the University of Pennsylvania*, attributes it to the Elamites about 2285 B. C. Later discoveries have shown that one Babylonian monarch at least did labor in the reconstruction of the temple of Bêl and has left inscribed bricks, and Hammurabi's code of laws, recently found by the French expedition at Susa, states distinctly that Hammurabi restored the temple. It would seem, therefore, that I was wrong, although it appears to be true that the temple of Bêl at Nippur was in general neglected during the period of the Hammurabi and Pashe dynasties of Babylon, and also during the time of Nebuchadnezzar and his successors. A fuller examination of the strata in which the broken objects were found seems to show that they were destroyed long before the period of the Elamite conquest of the country, nearer 2600 than 2285 B. C. Hilprecht, therefore, suggests (pp. 378, 379) that there must have been an Elamite invasion and sack of Nippur at that period, of which we have as yet no other record.

Hilprecht, as was to be expected, holds to the early date of Sargon of Akkad, 3800 B. C. He has found, in a pavement intervening between the pavements of Ur-Gur and Naram-Sin, inscribed bricks

<sup>12</sup> His representation of my view is not a fair presentation of the opinion actually held by me (Cf. *Nippur*, Vol. II, p. 262).

<sup>13</sup> Cf. *Nippur*, Vol. II, chap. 10.

with the short legend "Lugal-surzu, *patesi* of Nippur, priest of Bêl" (p. 476). But with this exception the period between those monarchs is still a complete blank, and, according to Hilprecht's account, there is in general no, or at best very little, intervening material between their constructions. The evidence at hand, so far as Hilprecht's account of the excavations at Nippur is concerned, seems to me to favor rather the date of 2800 B. C. than the earlier date advocated by him.

Hilprecht promises us shortly

a special work entitled "Ekur, the Temple of Bel at Nippur," which will be fully illustrated and accompanied by large plans and diagrams prepared by the architects of the expedition according to my reconstructions and their own survey of the actual remains still existing (p. 450).

Until that book appears it is impossible to criticise intelligently his restoration of the temple. He seems to have shown that the temple consisted of two courts (plan on p. 470), in the inner of which stood the *ziggurra*t and "the House of Bel,"<sup>14</sup> while in the outer were the small shrine of Bur-Sin, and, probably, a number of other similar shrines. Two gates of the inner court have been discovered, but the dimensions of neither court are yet ascertained. I sought for a temple gate at precisely the point at which Hilprecht locates the gate of the outer court of the temple in the text (his location of it in the plan is different), but, although I conducted excavations at this point to a very low level, I could find nothing. The point at which the gate is located in the plan corresponds, not with the apparent gate-like opening in the outer line of mounds, as Hilprecht says in the text, but with a curious tower-like construction in those mounds. Just without the shrine of Bur-Sin there is a mote-like depression running the whole length of the temple, and descending much below the level at which that shrine stands, which would seem to indicate that the outer limit of that court was not where Hilprecht places it. From his own account of the excavations and from my own experience, it seems to me that the dimensions of the inner court must have been, at least on the southwest side, different from those suggested in his plan; that there must have been more space and probably also buildings between the *ziggurra*t and the wall on that side. It may be that there is evidence, passed over in the description in the present volume, which will

<sup>14</sup>By the way, his method of exploring the latter building was precisely the same as my method of exploring the "Parthian palace," so severely condemned in this volume.



justify placing the southwest wall so close to the *ziggurat*. There are other points of which no notice is taken, like the great brick constructions at the north corner of the temple inclosure; and, indeed, it is evident, from Hilprecht's own account, that much work yet remains to be done before the temple can be reconstructed with any degree of certainty for any period of its history.

Hilprecht has discovered within the core of the *ziggurat* of Ur-Gur, the earliest which I discovered, a *ziggurat* of the time of the Sargonids, thus reversing his former view that *ziggurats* began with Ur-Gur. He believes himself to have found, also, evidence of a pre-Sargonic *ziggurat* (pp. 452 f.), which was "smaller than that of Naram-Sin and lay entirely within and largely below it." As he points out, from the fact that the names of the *ziggurat* and of the temple itself are Sumerian, we might expect to find the temple in existence in the pre-Sargonic period with a *ziggurat*, and it may be safe to conclude that temple and *ziggurat* both existed at that time, but it is not clear, from Hilprecht's account, that he has actually discovered a pre-Sargonic *ziggurat* in his excavations.

It seems that with the Sargonic period came a great change in the character of the temple, corresponding with a change in civilization and race. All the important structures yet discovered lie above the Naram-Sin pavement. Below that there is nothing but remains connected with the burning of the dead. And here we note a most striking change of custom, for the bodies of the dead, after the time of Sargon, were disposed of by burial, not by burning.<sup>25</sup> It is worth while, by the way, to compare with this result the somewhat similar result obtained by Macalister in the excavation of Gezer. He found at the lowest levels a pre-Semitic population of an extremely simple civilization disposing of their dead by burning. These were succeeded, somewhere, according to his estimate, between 3000 and 2000 B. C., by a Semitic population which practised burial instead of burning. The pre-Semitic population at Nippur, however, would seem to have been by no means so primitive and barbarous as the pre-Semitic population whose remains were discovered by Macalister at Gezer. The latter were troglodytes and made use of stone implements, and their pottery was of the rudest. In the pre-Sargonic remains at Nippur, on the other hand, were found fragments of lacquered pottery, black and red, more ornamental, according to Haynes's account, than the pottery

<sup>25</sup> Although, according to Hilprecht's account, the evidence for this at Nippur is negative rather than positive.

belonging to the later Semitic period (p. 406); as builders they had advanced so far as to understand the principle of the true arch, and during part of the period at least they practiced the art of writing.

The results of the discoveries in the pre-Sargonic period, as presented in this volume, are certainly most perplexing. Below the Sargon level, for a depth of thirty feet, there is nothing but remains of incinerations. The bodies of the dead were burned here and the ashes generally placed in jars, together with vessels containing food for the spirits of the dead. For men of greater importance, tomb chambers were built similar to those found by the German expedition at Zerghul and Hibba. There were wells, conduits, and drainage pipes to furnish water for the dead and drain the tombs and graves. What was supposed by Haynes to be an altar, Hilprecht now supposes to have been a sort of common pyre for the burning of the dead (pp. 395 and 453), but his arguments for this use are by no means conclusive. He suggests (pp. 459 f.) that the *ziggurra*t itself was originally of the nature of a tomb, as well as of a house for the gods, and that the burning of the dead was in a peculiar way connected with the sanctuary. It should be observed, however, that the excavations conducted on the west side of the canal to the same low level show precisely the same conditions (pp. 403 f., 419, 533). Below the level of Sargon nothing appears but the remains of incinerations, and those extend to a great depth below the level of the plain. The cremation of the dead would seem, therefore, to have been in no exclusive way connected with the temple. Another perplexing feature of these pre-Sargonic discoveries is the level at which they are found. The desert level at the present time is from six to eight feet below the Sargon level. The original plain level Hilprecht assumes to have been from ten and a quarter to eleven feet below the Sargon level. Virgin soil was thirty feet below the Sargon level and water thirty-five. The pre-Sargonic remains descend to virgin soil, that is, to a depth of thirty feet below the Sargon level, and almost twenty feet below what was then the level of the plain (pp. 391, 402). According to the results of Haynes's excavations in the canal bed (pp. 420 f.), the city level at that date would have been ten feet below the bottom of the canal bed. Altogether these results are quite inexplicable, and we must wait for further excavations to understand their meaning or even to be sure of the actual facts. Hilprecht, without giving his grounds, declares that the burials ascribed by me to the Babylonian period are in fact Parthian. There are, according to him, no Semitic burials at Nippur.

We pass from the burnings of the pre-Sargonic period to the interments of the Parthian period, with nothing between. This sounds incredible.

One of the interesting discoveries made by Professor Hilprecht, not through excavation, but by interpretation of inscriptions, is the identification of the canal which divides Nippur into two parts, the great Shatt-en-Nil, which leaves the Euphrates at Babylon and joins it again in the neighborhood of Erech, with the river Chebar, by which the Jewish exiles were settled (p. 411); and he also suggests that Tel-Abib (or Tel Abub?) was at or in the neighborhood of the sand hills three or four miles to the northeast of Nippur.

Of the situation of the library I have already spoken. Hilprecht locates the business quarter along the canal on its west side (pp. 413, 414). I have already stated that a large number of business documents were found to the east of that canal in the southeast or "library" hill, while considerable numbers of tablets belonging to the temple were found at various points on the west of the canal, together with some literary documents. I do not think that, from the excavations so far conducted, it is possible to determine with any degree of certainty the topography of the city. The greater part of the west side of the canal is quite unexplored, and the same is true of the east side of the canal, outside of the temple mound, "Tablet Hill," and a part of the outer wall. It is to be hoped that the work of excavating Nippur will be carried forward to a real conclusion. Only a small part of its vast mounds has yet been examined, and no one part has been completely excavated. The results in our hands up to the present moment are, therefore, most fragmentary.

In the end, doubtless, the inscribed objects found will prove to be of the greatest value and of the greatest interest. Hilprecht, in his notice of objects found in the "library," has given a most fascinating picture of what the full decipherment of those tablets must yield in the educational line alone: school exercises, multiplication tables, and the like; the evidences of careful mental and manual discipline, the processes of education in the third millenium B. C.; a school of art; even an interest in archæology, represented by a little collection of valuable antiquities, including the fragments of a "ground plan of the environments of Nippur" (p. 548), which we hope may be more fully explained in a later work. But Hilprecht also points out that the texts so far found in the library are mathematical, astronomical, astrological, linguistic, grammatical, and, to some extent, religious, and

that the texts from Nippur, copied by the scribes of Ashurbanipal for his library, included just these categories, excluding the religious one. The question arises, therefore: Will the library finds at Nippur give us only material of these classes, or may we also expect literary material, such as the epic of Gilgamesh, including the flood legend, creation legends, etc.?

It is deeply to be regretted that the policy hitherto pursued has prevented a more speedy publication of the contents of inscribed tablets. Practically all these collections have been retained exclusively in the hands of Professor Hilprecht and his pupils. The result of such a course must be a long delay before the contents of the "library" and the other collections of tablets can be properly communicated to the world, the more so as only a portion of Professor Hilprecht's time, apparently, is to be henceforth devoted to the copying and decipherment of this material. It is to be hoped that the University of Pennsylvania will invite the co-operation of Assyriologists in the study and interpretation of the precious documents from Nippur.

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#### WHERE MAY CHRISTIAN CERTAINTY BE FOUND?

THE first of these two works,<sup>1</sup> whose author has since been called from Erlangen to succeed Luthardt in the chair of dogmatics at Leipzig, is a thoroughgoing treatment of its subject, the principal defect of which is a somewhat unnecessary prolixity of discussion. A pupil of Frank, and in general occupying the same dogmatic and confessional position, Professor Ihmels was early led to question whether in Frank's treatment of "Christian Certainty," the Scriptures received their due, and whether certainty as to the new birth should be made the central certainty of the Christian, as was done by Frank. The present work attempts to answer these queries.

One hundred and sixty-seven pages are devoted to "an historical orientation," in which Luther, the old-Lutheran dogmatics, the period from Pietism to Supranaturalism, Frank, and Herrmann are successively reviewed. The remaining portion is devoted to a connected

<sup>1</sup> *Die christliche Wahrheitsgewissheit, ihr letzter Grund und ihre Entstehung.* Von L. IHMELS. Leipzig: Deichert, 1901. vi+344 pages. M. 5.60.

*Neue Grundlegung der Lehre von der christlichen Gewissheit.* Von ALEXIS SCHWARZE. Göttingen: Vandenhoeck & Ruprecht, 1902. ii+189 pages. M. 3.80.